Associate Editor

Thank you for submitting a scientifically interesting and robust modelling study exploring how hydroclimatic conditions influence streamflow generation and plant water uptake processes and resulting water ages of the different fluxes and stores. The reviewers state a need to extend the literature covered in the Introduction and Discussion and provide several examples of useful peer-reviewed studies. The reviewers also request more justification in the modelling choices as well as clearer definitions of the terminology used. I agree with these recommendations, and in particular there is utility in considering and contrasting the precipitation regimes (e.g., snow dominated, rain dominated) and topographic/critical zone context (e.g., steep mountainous systems, thin, or highly fractured subsurface) for the studies cited. In addition, I would encourage you to revisit the design of many of your figures to make them easier to digest. For example, it is very challenging to see the differences in months in Figure 6. Perhaps consider stacking the monthly distribution plots on top of each other and make them slightly transparent. That will help showcase the differences (and similarities) between months.

Given the reviewer recommendations as well as my identification of making the visualizations more accessible to readers, I recommend this manuscript get resubmitted after major revisions. Thank you and I look forward to receiving your revised manuscript.

We thank the Associate Editor for the careful evaluation of our manuscript and for the constructive summary of the reviewer comments. Following these recommendations, we have substantially revised the manuscript. Specifically, we expanded the literature review and discussion of hydroclimatic and geomorphic controls, clarified model assumptions and terminology, added governing equations and supporting parameter information, revised figure captions throughout the manuscript, and improved the presentation of several figures (including Figure 6) to enhance readability. Detailed point-by-point responses to all reviewer comments are provided below.

We appreciate the Associate Editor's suggestion regarding the presentation of Figure 6. We considered alternative layouts, including stacking monthly distributions. However, because Figure 6 involves multiple months and multiple forcing scenarios, separating the distributions into many smaller panels would improve some month-to-month comparisons but would make it more difficult to follow the intra-annual evolution within each scenario and to compare the overall seasonal patterns across scenarios. We therefore retained the current scenario-based layout and revised the figure by adding annotations and expanding the caption to more clearly guide interpretation and highlight the key similarities and differences among scenarios and months.

Reviewer #1

I have revised the paper entitled ‘Progressive groundwater decoupling may drive a shift toward shallower and faster terrestrial water cycling’. The paper is well-structured, scientifically sound, and can be published in the Hydrology and Earth System Science journal after some revisions. These can be itemized as follows. Best regards.

We thank the reviewer for the careful evaluation of our manuscript and for the constructive comments and suggestions. We found these comments very helpful and believe they have substantially improved the clarity and quality of the revised manuscript. Below, we provide a point-by-point response to each comment. All line numbers cited in our responses refer to the revised manuscript with tracked changes enabled.

- There is a significant portion of the literature that may be useful to cite in the work. The Authors wisely underline key issues, like the fact that groundwater regulates surface fluxes IF it acts as long term reservoir. But the roots of these reasonings are deeper than data-driven studies (Condon et al., 2020; Miguel-Macho and Fan, 2025) and needs to be framed. In this context, the works of Berkowitz and Zehe (2020) and Schiavo et al. (2022) are milestones in reshaping the mutual role of surface and groundwater, being mutually feeding and more similar than we think. Berkowitz and Zehe (2020) assessed the similar nature of the two ‘water worlds’, while Schiavo et al. (2022) were the first to implement gravity-driven behaviors in groundwater networks to highlight the similar self-organized structure of groundwater flow patterns. These two works were important in closing the ties between these two worlds.

We revised the Introduction to incorporate Berkowitz and Zehe (2020) and Schiavo et al. (2022), which emphasize the interconnected nature of surface water and groundwater and challenge the traditional separation of the two “water worlds”. Please refer to lines 37–40.

- Recent works have highlighted the water-limited nature of mountain aquifers. This has been clearly stated by Betterle and Belling (2024) for headwater catchments, whose discharge patterns. This is due to the geomorphological forcing being the main responsible for drainage patterns in prealpine and alpine environments. Moreover, they also have assessed the same three zones of different groundwater dynamics behaviors. I suggest incorporating these references as well to better frame the present conceptualization, also offered in Figure 1, in the recent literature.

We incorporated Betterle and Bellin (2024) into the revised manuscript and revised the Introduction and Figure 1 caption to better connect our conceptual framework with recent studies on groundwater connectivity and water-limited catchments. Please refer to lines 52–65 and the revised Figure 1 caption.

- I am a bit skeptical about the deterministic values fixed as parameters in lines 100-111.

Can the authors justify this choice and explain why they did not go for intervals of confidence for these parameters? The whole Section 2 should be sustained in the light of this comment.

The objective of this study was not to identify a unique parameter set or quantify parameter uncertainty, but rather to examine a connectivity-driven reorganization mechanism using a representative reference configuration adopted from previous ParFlow–EcoSLIM studies (Maxwell et al., 2019; Yang et al., 2022). To evaluate whether the identified tendency depends on a specific parameter choice, we further conducted additional experiments with modified vegetation type, subsurface hydraulic properties, and hillslope gradient. Although the magnitude of the response varied, the qualitative pattern remained evident across all tested configurations. Please refer to Methods (lines 147–152, 216–224, and 228–242) for the reference configuration and additional parameter configurations, and to Section 3.4 (lines 480–506) for the corresponding results.

- At lines 142-145, the Authors say they reduced the topographic gradient as lateral subsurface drainage control. Why did they do that? Is that consistent with local geological, hydrogeological, and flow patterns features? Saying this, they basically say the local flow patterns (lateral subsurface drainage) are being altered. This should be strongly supported. Moreover, wouldn't these lateral drainage patterns change on a monthly basis rather than being just flattened a priori?

The reduced hillslope gradient was introduced as a controlled perturbation to examine how weaker topographic control on lateral drainage influences the proposed mechanism. It was not intended to represent a temporal change in topography or a specific field condition. As clarified in the revised Methods section, the objective of these experiments is to evaluate the robustness of the identified tendency across contrasting system configurations rather than to reproduce a particular catchment. Please refer to lines 216–224 and 228–242. We also added Table 1 to clarify the experimental design and the sixteen scenario combinations.

- Figure 6 is not clear to me. What is there on the y-axis? Could the authors find a better way to show it? Which core info do they want to share with this Figure?

To improve the clarity of Figure 6, we revised both the figure and the caption. Specifically, we added additional annotations within the figure to help guide interpretation. We also expanded the figure caption to provide a clearer explanation of the y-axis definition and to more explicitly describe the main message conveyed by the figure. Please refer to the revised Figure 6 and its updated caption.

- When introducing the parflow model, such as the surface flow model and subsurface unsaturated flow model, the Authors should introduce a small section with fundamental governing equations.

Added. Please refer to line 157 and Text S1 in Supplementary Information.

References:

Berkowitz, B. and Zehe, E.: Surface water and groundwater: unifying conceptualization and quantification of the two “water worlds”, Hydrol. Earth Syst. Sci., 24, 1831–1858, https://doi.org/10.5194/hess-24-1831-2020, 2020.

Betterle, A., & Bellin, A. (2024). Morphological and Hydrogeological Controls of Groundwater Flows and Water Age Distribution in Mountain Aquifers and Streams. Water Resources Research, 60(11), e2024WR037407. https://doi.org/10.1029/2024WR037407

Schiavo, M., Riva, M., Guadagnini, L., Zehe, E., & Guadagnini, A. (2022). Probabilistic identification of Preferential Groundwater Networks. Journal of Hydrology, 610, 127906. https://doi.org/10.1016/j.jhydrol.2022.127906

Reviewer #2

Review for "Progressive groundwater decoupling may drive a shift toward shallower and faster terrestrial water cycling" by Sun et al., for consideration in HESS.

This is an interesting study that is nicely organized and concisely presented. I believe it fits the scope of HESS and will be fit for publication after some clarifications and revisions. I have listed general and specific comments below that I hope will strengthen the paper.

We sincerely thank the reviewer for the thorough review and valuable suggestions. The comments have helped us improve both the presentation and interpretation of our results. We have carefully considered each point and revised the manuscript accordingly. A detailed point-by-point response is provided below. All line numbers refer to the revised manuscript with tracked changes enabled.

General Comments:

1. The introduction relies heavily on two studies— one of which focuses on a snow-dominated mountain catchment. It should be noted that mountain sites like East River can have many streamflow generation processes and are shaped by other influences that are not discussed (e.g., snow, steep topography, shallow soils, mountain-block recharge, etc.—see Jencso et al., 2009; Somers and McKenzie, 2020) and may not be relevant elsewhere. How do these characteristics factor into the proposed shift toward shallower water cycling? Are there landscapes in which this shift would be unlikely to occur? If so, I would suggest to be more explicit in the scope of where this work is applicable.

- References:
- Jencso, K. G., et al. (2009). "Hydrologic connectivity between landscapes and streams: Transferring reach- and plot-scale understanding to the catchment scale." Water Resources Research **45**(4).
- Somers, L. D. and J. M. McKenzie (2020). "A review of groundwater in high mountain environments." WIREs Water **7**(6).

We thank the reviewer for this thoughtful comment. We agree that mountain catchments such as the East River are influenced by additional processes, including snow accumulation and melt, steep topography, shallow soils, and mountain-block recharge, which are not explicitly represented in our idealized hillslope framework.

Our objective was not to reproduce a specific catchment, but rather to isolate a connectivity-driven reorganization mechanism under increasing hydrologic stress. To evaluate the robustness of this tendency, we conducted additional experiments with modified vegetation type, subsurface hydraulic properties, and hillslope gradient (Section 3.4). Although the magnitude of the response varied, the qualitative pattern of age-gap formation and deep–shallow decoupling remained evident across all tested configurations. Please refer to lines 480–506. We also revised the Introduction to reduce the emphasis on the snow-dominated East River catchment and clarify that the proposed mechanism is not intended to represent a specific mountain environment or snow-driven process. Please refer to lines 52–56.

At the same time, we agree that natural catchments involve simultaneous variations in climate, vegetation, topography, and subsurface structure, and that additional processes such as snow accumulation and melt or mountain-block recharge may amplify, suppress, or otherwise modify the manifestation of the mechanism identified here. We therefore added a new paragraph at the end of Section 4 discussing catchment heterogeneity, unrepresented processes, and the need for future evaluation in real-world systems. We also note that seasonal snow storage may delay water release and sustain recharge during dry periods, potentially buffering the development of age segregation and deep–shallow decoupling. Please refer to lines 554–565. The suggested references have also been incorporated into this discussion.

2. The term “reorganization pathway” is used several times but is not very clear, please define with first use. Does this suggest that under dry conditions, when the water table has receded to some critical point, a preferential pathway for young/local water emerges?

Clarified. Please refer to lines 112–115 and 66–68.

3. I would suggest to briefly summarize the main message from each figure into its respective caption to help direct the reader’s attention.

We have revised the figure captions throughout the manuscript to briefly highlight the main message of each figure and guide the reader’s interpretation of the key results. Please refer to the revised captions of all figures.

4. How would the findings from this study be extrapolated to real-world conditions (e.g., more heterogeneity in hillslope parameters)? I see that expanding to real study sites is potential future work, but some discussion of study limitations would be helpful to ground the broader applicability of this work’s conclusions.

We thank the reviewer for this comment. Following this suggestion, we added a paragraph at the end of the Discussion acknowledging the limitations of the present idealized framework and noting that additional processes and catchment heterogeneity may influence the manifestation of the proposed mechanism in real-world systems. Please refer to lines 554–565. Additional discussion of this issue is provided in our response to Comment 1.

Specific Comments:

Line 41: Suggest to revise the following to be more precise: “…as shallow soil moisture is depleted, “relative” groundwater contributions increase…”.

Edited as suggested. Please refer to line 47.

Line 66: Suggest to revise wording: I would not say the expectation is “strengthened” groundwater buffering under intensifying drought, rather more of a shift toward reliance on groundwater stores in the absence of input precipitation. The wording used later “intensifying groundwater dependence” seems more accurate to me.

Edited as suggested. Please refer to lines 69 and 575.

Figure 1: Should the “local” water still be connected to the stream in panel b to show how the shallow flow paths dominate the water contributions?

Revised. Please see the new Figure 1.

Lines 107-111: Are there references to support these specific parameter values?

We highlighted the references in revision. Please refer to lines 147–152 and 221.

Line 112: It would be helpful to include the key governing equations for the ParFlow and EcoSLIM models.

Added. Please refer to line 157 and Text S1 in SI.

Line 115: I realize that the aim is not to represent a specific field site, but Oklahoma is a fairly large and climatologically diverse state. Can you be more specific about where in Oklahoma the meteorological record is from?

Revised. Please refer to line 158.

Line 134: What LAI values were used in the baseline configuration? Perhaps a table in SI summarizing the key parameters for the baseline and additional scenarios would be helpful to include. I wonder also whether the perturbation scenarios are realistic for the “semi-arid woody hillslope” being represented in the baseline case? Is the “greening” scenario due to CO2 fertilization or some other driver? The relevance of these additional scenarios would benefit from more explanation and literature support.

We thank the reviewer for this helpful suggestion. We have added Table S1 in the Supplementary Information summarizing the LAI values used in the baseline and perturbation scenarios. Please refer to Table S1.

We also expanded the description of the LAI perturbation, provided additional justification for the selected magnitude, and incorporated additional literature support (Chen et al., 2019; Piao et al., 2020). Please refer to lines 206–215.

The greening scenario is intended to represent an increase in vegetation abundance rather than a specific underlying driver. Accordingly, our focus is on the hydrologic consequences of vegetation expansion, regardless of whether it arises from CO_2 fertilization, climate change, land management, or other processes.

Figures 2 & 3: What is the variation associated with the monthly values? If the model was run for 20 years at an hourly timestep, I assume the monthly values shown are averages.

ParFlow–CLM was first spun up for three years, and the third year was used for the water-balance analysis shown in Figure 2 and to initialize a subsequent 20-year EcoSLIM particle-tracking spinup. Figure 3 was based on the final year of the EcoSLIM particle-tracking simulation. All simulations were performed at an hourly timestep, and the monthly values shown in Figures 2 and 3 represent monthly averages calculated from the hourly outputs of the corresponding analysis year. Please refer to the Methods section (lines 153–205) and the revised figure captions.

Figure 3: Why are there no points in September for the non-baseline scenarios? And no point in August for the red line scenario (Temperature+1K MaxLAI+50%)?

In these months, groundwater–surface decoupling resulted in zero outflow at the hillslope outlet, and therefore no outflow age could be computed. The absence of these points is itself a manifestation of the decoupling process discussed in the manuscript. We have clarified this in the revised figure caption.

Figures 4 and 5: It is a bit confusing to have the "x" direction of the hillslope on the vertical axis of the plots. It may also help to add a small panel with the hillslope configuration for visual context (e.g., as in Figure S3).

To improve the clarity of Figures 4 and 5, we revised the axis labels and annotations to more explicitly indicate the spatial orientation of the hillslope. Specifically, we now identify the toe (0 m) and upslope (100 m) positions and consistently use "Location along hillslope" to describe the spatial coordinate. We also expanded the figure captions to clarify the hillslope orientation and facilitate interpretation of the spatial patterns shown in the figures. Please refer to the revised figures and captions.

Line 244: Does the "lower 20m" refer to the "x" direction of the hillslope? (A visual representation of this would be helpful to avoid confusion).

Yes, the "lower 20 m" refers to the downslope portion of the hillslope in the x-direction. To make this clearer, we added a highlighted box in Figure S3 indicating the corresponding location along the hillslope. Please refer to line 327 and the revised Figure S3.

Line 247: I believe this is the first use of "age gap"—can this be defined explicitly here?

We now explicitly define the term "age gap" at its first use in the revised manuscript. Please refer to lines 353–355.

Figure 6: It is difficult to see the differences between scenarios in this figure. Would it be helpful to overlay the months for different scenarios or add some guiding labels/annotations to highlight what the main message of the figure is? (Summarizing the main message in the caption as suggested above would be helpful, as well).

To make the differences among scenarios easier to identify, we revised both the figure and the caption. Specifically, we added additional annotations within the figure to guide interpretation and highlight the key patterns. We also expanded the figure caption to more explicitly summarize the main message conveyed by the figure. Please refer to the revised Figure 6 and its updated caption.

Figure 7: Similar to the comment about Figures 4 and 5, it is confusing to have the "x" direction of the hillslope represented on the vertical axis of the right panels, but then on the horizontal axis of the left panels. It would also help to add some description for the blue rectangles in the x vs. ET age panels in the caption.

Similar to Figures 4 and 5, we revised the axis labels and annotations in Figure 7 to more clearly indicate the hillslope orientation and improve the consistency of the spatial coordinates shown in the different panels. We also expanded the figure caption to explain the meaning of the blue rectangles in the location–ET age panels and to facilitate interpretation of the figure. Please refer to the revised Figure 7 and its updated caption.

Line 337-339: This statement needs stronger support—what is the evidence that old water in streams is sourced from deeper soil moisture as opposed to groundwater? (And how is "deep soil moisture" defined here?)

Edited as suggested. Please refer to lines 517–520.

Line 339-341: Suggest to revise wording: As with previous comments (lines 41, 66), I think it would be more accurate to say that there is a greater reliance/dependence on groundwater under increasing stress (at least following the typical conceptualization), rather than becoming "more engaged".

Edited as suggested. Please refer to lines 520–522.